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HUMAN RADIATION STUDIES: REMEMBERING THE EARLY YEARS

Oral History of Health Physicist
Karl Z. Morgan, Ph.D.

Conducted January 7, 1995

United States Department of Energy
Office of Human Radiation Experiments
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FOREWORD

In December 1993, U.S. Secretary of Energy Hazel R. O'Leary announced her Openness Initiative. As part of this initiative, the Department of Energy undertook an effort to identify and catalog historical documents on radiation experiments that had used human subjects. The Office of Human Radiation Experiments coordinated the Department's search for records about these experiments. An enormous volume of historical records has been located. Many of these records were disorganized; often poorly cataloged, if at all; and scattered across the country in holding areas, archives, and records centers.

The Department has produced a roadmap to the large universe of pertinent information: *Human Radiation Experiments: The Department of Energy Roadmap to the Story and the Records* (DOE/EH-0445, February 1995). The collected documents are also accessible through the Internet World Wide Web under `http://www.ohre.doe.gov`. The passage of time, the state of existing records, and the fact that some decisionmaking processes were never documented in written form, caused the Department to consider other means to supplement the documentary record.

In September 1994, the Office of Human Radiation Experiments, in collaboration with Lawrence Berkeley Laboratory, began an oral history project to fulfill this goal. The project involved interviewing researchers and others with firsthand knowledge of either the human radiation experimentation that occurred during the Cold War or the institutional context in which such experimentation took place. The purpose of this project was to enrich the documentary record, provide missing information, and allow the researchers an opportunity to provide their perspective.

Thirty audiotaped interviews were conducted from September 1994 through January 1995. Interviewees were permitted to review the transcripts of their oral histories. Their comments were incorporated into the final version of the transcript if those comments supplemented, clarified, or corrected the contents of the interviews.

The Department of Energy is grateful to the scientists and researchers who agreed to participate in this project, many of whom were pioneers in the development of nuclear medicine. ❑

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DISCLAIMER

The opinions expressed by the interviewee are his own and do not necessarily reflect those of the U.S. Department of Energy. The Department neither endorses nor disagrees with such views. Moreover, the Department of Energy makes no representations as to the accuracy or completeness of the information provided by the interviewee.

ORAL HISTORY OF HEALTH PHYSICIST KARL Z. MORGAN, Ph.D.

Karl Z. Morgan was selected for this oral history project because of his research for the Manhattan Project at the Metallurgical Laboratory in Chicago, and his work at the Oak Ridge National Laboratory (ORNL) in Oak Ridge, Tennessee. The oral history covers Dr. Morgan's work as a pioneer in the field of health physics, his health physics research at the Oak Ridge National Laboratory, and his work since he retired from ORNL.

Short Biography

Dr. Morgan was born in Enochsville, North Carolina on September 27, 1907. He attended Lenoir-Rhyne College (Hickory, North Carolina), received B.S. and M.S. degrees (in Physics and Mathematics) in 1929 and 1930, respectively, from the University of North Carolina, and received his Ph.D. (Cosmic Radiation) in 1934 from Duke University (Durham, North Carolina). He is married and has four grown children.

Dr. Morgan began his career as a physics professor at Lenoir-Rhyne College (1934–1943), where he focused his work on cosmic ray research. In 1943, Dr. Morgan moved to Chicago to become a senior scientist in health physics for the Manhattan Engineer District. The following year, Dr. Morgan went to the newly formed Oak Ridge National Laboratory (formerly Clinton Laboratories) in Oak Ridge, Tennessee, where he served as Director of Health Physics from 1944 to 1972.

Since joining ORNL, Dr. Morgan has also held the following positions:

- 1945 to '71—Member, International Commission on Radiological Protection (ICRP)
- 1955 to '78—Member, National Council on Radiation Protection (NCRP)
- 1955 to '78—Editor-in-Chief, *Health Physics Journal*
- 1960 to '72—Adjunct Professor of Health Physics, Vanderbilt University
- 1972 to '83—Professor of Health Physics, Georgia Institute of Technology
- 1983 to '86—Visiting Professor of Health Physics, Appalachian State University (Boone, North Carolina).

Dr. Morgan has published many times on health physics, addressing such topics as the maximum permissible occupational dose of specific isotopes. In addition, Dr. Morgan has researched and published numerous articles dealing with nuclear worker safety issues.

College and Graduate School in North Carolina; Unintentionally Joining the Manhattan Project in Chicago in 1943

CAPUTO: Today is January 7, 1995. My name is Marisa Caputo and I'm here with Michael Yuffee. We are from the Department of Energy's Office of Human Radiation Experiments. We're here today in Indian Springs, Florida, to interview Dr. Karl Morgan about his knowledge of Cold War human radiation experimentation. Dr. Morgan, I was hoping that we could start with where you were born and the year you were born, and then maybe get into your educational background.

MORGAN: I am Karl Ziegler Morgan. I was born in a small village—Enochsville, North Carolina, not far from Charlotte—on September 27, 1907. I spent a good portion of my early life in Raleigh, the later portion in Salisbury, North Carolina. I spent the first two years of college at a small college, Lenoir-Rhyne, in Hickory [North Carolina]. I then went to the University of North Carolina, where I received my B.S. and M.S. in Physics and Mathematics.

I worked about a year with Westinghouse in Philadelphia in 1930. Then I received a fellowship at Duke University, where I received my doctorate in 1934. My major research project there was the study of cosmic radiation.[1] Following my receipt of the Ph.D. degree—this during the Depression—I became chairman of the Physics Department at Lenoir-Rhyne College in Hickory.

While teaching, I did cosmic ray research for about eight years. Research in caverns way underground and high mountains, Mt. Mitchell, Mt. Evans in Colorado, etc.

YUFFEE: Well, after your tenure with Lenoir-Rhyne, you went to the University of Chicago and joined the MED?[2]

MORGAN: While doing research at Lenoir-Rhyne, with Drs. [Walter] Nielsen and others there, one of the research projects was in Dr. [Arthur] Compton's[3] laboratory on top of Mt. Evans [in Colorado]. During the summer I was there, I met Dr. [Jason] Sterns, chairman of the Physics Department at

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1 radiation of high penetrating power originating in outer space and consisting partly of high-energy atomic nuclei

2 Manhattan Engineering District, the Government agency that oversaw development of the atomic bomb under the ultrasecret Manhattan Project

3 Dr. Arthur Compton of the University of Chicago headed a National Academy of Sciences committee that in May 1941 recommended to Dr, Vannevar Bush, head of the National Defense Research Committee, that nuclear research be pursued as part of the national defense effort for several purposes, including development of an atomic bomb. In the summer of 1941, Bush instructed Compton to assess technical questions related to critical mass and destructive capability and verify a British conclusion that development of a uranium bomb that could be dropped from existing aircraft was feasible within two years. On November 6, 1941, Compton reported a conclusion less sanguine than that of the British but still confirming the feasibility of an atomic weapon deliverable by aircraft. Early in 1942, as part of the emerging effort to develop an atomic bomb, Bush appointed Compton to be one of three program chiefs with responsibility to run chain reactions and develop weapons theory. As a result, under Arthur Compton, the Metallurgical Laboratory at the University of Chicago became a critical research facility for the Manhattan Project.

the University of Denver, and by accident, I casually mentioned my desire to find a job out West because I was fond of the mountains. Dr. Sterns was excited, and he said he'd always wanted to come East. So we then planned to change our positions. He was going to take my chairmanship at Lenoir-Rhyne and I was to take his in Denver. We exchanged a rather intensive correspondence, and then suddenly, during the winter of [early] 1943, the correspondence ceased. I heard nothing more from him. I assumed he had lost interest in moving East for some unknown reason.

Then suddenly I had phone calls from Sterns, Compton, and others in Chicago, urging me to come to Chicago because there was an extremely exciting program there that related to my cosmic ray research. For example, I was the only person in the [Southeast] part of the U.S. that had ever built and used cosmic ray [detecting] circuits. That was one of the principal instruments that was to be used in measuring the ionizing radiation.[4]

Well, I was still peeved with Sterns for not having answered my correspondence. After a few weeks and discussion with [Walter Nielsen and] Lother Nordheim, the theoretical physicist I was working with at Duke, [I made up my mind]. Incidentally, we [(Nordheim, Nielsen, and I)] helped to identify and discover the third particle of matter, the meson.

Nielsen said, "Well, maybe Dr. Compton and Sterns and others are trying to obtain energy from the atom, and maybe they could use that for a weapon." Well, eventually my curiosity got the best of me, and I found myself on the train going to Chicago. People did not go by plane in that period.

Chosen for the New Field of Health Physics (1943)

MORGAN: After my preliminary clearance, I walked into Compton's office, and Dr. Sterns and others were there. Sterns said, "Well, Karl, you'll be in the Health Physics Group." I was very much shocked and started toward the door. I said, "This is a terrible mistake: I've never even heard of health physics." Sterns said, "Hold on, Karl, we'd never heard of it ourselves till a few weeks ago. We have a very difficult problem: We are going to have intense sources of radiation and we believe that it's a problem of physics, primarily, to protect people from this radiation. So we are forming a small group under Dr. E.O. Wollan."

So I calmed down. Dr. Robert [S.] Stone was there; he was the Associate Director for Health under Arthur Compton. Bob said that they were determined, he and Arthur, to do this work safely. He reminded me that the radium dial painters had suffered [serious] consequences with radium. [He said] that [if] they were able to gather together all the radium in the world from physics labs and hospitals—at that time, it would be

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[4] radiation that interacts with matter by stripping electrons from their orbits around nuclei, leaving the nucleus with a positive charge

about two pounds; say, the size of a golf ball. Radium is quite dense, atomic number 226. They were going to build piles (instead of reactors). They called them piles, piles of graphite [and] uranium, primarily in those days, in which the intensity of ionizing radiation would not be equivalent to all of the available radium in the world [(two pounds)], but millions of billions times higher. They were going to surround these monsters with thick concrete, six feet or more, to protect people on the outside, and he and Dr. Compton were determined to do this work safely.

YUFFEE: Had they already told you, at this point, about [Enrico] Fermi and his reactor?

MORGAN: Well, no, I did not know about the Fermi reactor under the athletic stands[5] during my first few weeks in Chicago, until my preliminary clearance was completed. Bob Stone went on to emphasize that [Otto] Hahn, [Fritz] Strassmann, [Lise] Meitner, and others in Germany were the first to discover the fission of uranium. [These Germans] had carried on some rather extensive studies, and the Chicago group was confident that Hitler and his associates were hellbent on developing a nuclear weapon, and that we were far behind. Stone and Compton were determined to catch up, if possible; but at the same time, to do the work safe from exposure to radiation.

Determining Safe Doses for Ionizing Radiation at Chicago (1943)

YUFFEE: How did you go about determining what exposure would be safe during your year[6] with the MED in Chicago?

MORGAN: Well, I don't know whether we ever determined that it was safe. [I try to address this question in *The Angry Genie*, a book I'm writing]. We determined what we considered acceptable. During the first months of my stay in Chicago, there were five of us: E.O. Wollan, the head of the group; Herbert Parker, an Englishman who had been working with Simeon Cantrill in Seattle; Carl Gamertsfelder,[7] a young doctoral student from Washington University [in St. Louis]; myself; and a little later, Jim Hart, a DuPont chemist. There were others that joined the group for short periods of time, but these five were the ones that lasted at least until we got to Oak Ridge [Clinton Laboratory, Oak Ridge, Tennessee].

Well, how did we determine the [nature of our] problem? We read intensively all the literature related to radiation exposure and consequences. When I say "ionizing radiation," I mean primarily that above about 15

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5 Stagg Field at the University of Chicago. Fermi's underground labs would become the site of the world's first sustained nuclear chain reaction, December 2, 1942.

6 Morgan actually spent less than 10 months in Chicago, ending in September 1943.

7 For the transcript of the interview with Gamertsfelder, see DOE/EH-0467, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Carl C. Gamertsfelder, Ph.D.* (scheduled to be published later in 1995).

electron-volts.[8] As you know, visible light is on the order of one to three or four electron-volts, and here we were at a level of 15 electron-volts or more, sufficient to ionize, or pull or push electrons out of the atom and produce ion pairs. Our study was to find the effects of this ionizing radiation on man and his environment.

There wasn't much in the literature that was helpful. All we found, essentially, might be listed under the meager information on the speculation that radium dial painters had a higher instance of cancer than would normally be expected. [It resulted from] tipping of the brushes [with their lips], pointing of the brushes they dipped in radium paste when they painted the dials of watches.

The other [main source of information] was a fair amount of data—a few scores of papers—that related to skin erythema.[9] The most common unit of ionizing radiation at that time was the *erythema dose*. Most of the literature was in the medical journals, relating to the problems that dentists and radiologists, primarily, had had when their hands became red and painful, and it was considered to be the same as [the "sunburn"] you get from extensive ultraviolet [radiation] exposure. So, the first period at Chicago was spent in trying to determine what levels would be acceptable for workers and for the public, and in the development of instruments that could be worn on the person of the worker, and that could be carried by surveyors and could be displayed in the environment and working areas.

We had to find out the risks of beta[10] and electron radiation relative to x radiation. We had no data on gamma radiation.[11] We supposed it would be similar to the equivalent energy of x rays and we had a little information, as I indicated, on alpha [radiation,][12] which was, of course the radium dial studies. We had absolutely no information on the effects of neutrons—fast epithermal or thermal neutrons.

So this was a horrendous task, to try to read all that was available. We attended numerous seminars. We gave some and we listened to many, from others in various departments in the university and those that were working on the Manhattan Project—that was a code name used at the time for our work under Compton.

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8 (eV)—a unit of energy equal to the energy acquired by an electron accelerating through a potential difference of 1 volt and equivalent to 1.602×10^{19} joules

9 an abnormal reddening of the skin due to local congestion, such as inflammation

10 an electron or positron emitted from an atomic nucleus in beta decay. Uranium emits beta particles because its beta-emitting decay products are present.

11 a highly penetrating photon of high frequency, usually 10^{19} Hz or more, emitted by an atomic nucleus

12 a positively charged particle consisting of two protons and two neutrons, emitted in radioactive decay or nuclear fission; the nucleus of a helium atom

Developing New Dosimetry Instrumentation

MORGAN: Dr. Wollan spent most of his time developing fiber dosimeters. They're small electrometers with a fiber that moves across the scale proportional to the dose administered to the instrument.

Hart, Parker, and Gamerstfelder spent a good bit of their time on "pencil dosimeters." They are small electrical condensers, air condensers. In physics units [they had a compacity] of one to two [cubic] centimeters.

I spent a major part of my time, in addition to learning what the radiation health problems were, in studying neutron exposure and development of instruments to measure neutron dose.

Dr. Gamerstfelder and I, along with some help from Parker, developed what we called a "chang and eng." [This instrument consisted of] two small cylinders; one was filled with nonhydrogenous[13] gas, like argon, and the other with gas like hydrogen or methane [(CH_4)]. As you know, neutrons don't produce ionization along their path because they have no charge, and their only ability to cause ionization is when they strike one of the nuclear components—that is, a proton or a neutron or a collection of nuclear particles.

So, with two chambers—one filled with gas containing hydrogen, the other with no hydrogen—and having them under pressure to give a large cross-section, we measured the differential output of these two chambers. I could measure [accurately] the neutron contribution from fast neutrons. Now, these chambers were very effective and very quantitative in their evaluation, so I used them later in some experiments in Oak Ridge.

Well, we developed many other instruments. I was a principal advocate for the use of Geiger counters.[14] No one [except a handful of cosmic ray physicists] had ever heard of them before. I later regretted having introduced them without sufficiently warning about their shortcomings. As you may know, in high, intense radiation, the pulses come too close together [and can't be resolved, so the counter reads zero]. [In spite of this], they were, and still are, one of the most, if not *the* most, commonly used and useful instrument in measuring ionizing radiation, since they are more sensitive than most other devices used [and extremely simple in their operation].

YUFFEE: How did you test these instruments? Were there animal studies? Were there studies with people? Or were you just leaving the instruments exposed to a source to see whether they worked or not?

MORGAN: [While at Chicago,] we did no studies in biology or in the environment. That was left up to other groups at the University. We tested these instruments, with radium sources and [chang and eng and] beryllium sources of neutrons. Then, if we wanted to study epithermal or thermal

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13 not containing hydrogen

14 instruments for detecting ionizing radiation and measuring dose rate

[neutrons,] we used paraffins[15] or something to slow down the neutrons. But, the biggest problem then, and even now, is that of the fast neutrons.

Arrival at Oak Ridge (1943)

MORGAN: Perhaps I can skip a few months, until we reached Oak Ridge.

CAPUTO: You went to Oak Ridge in 1944?

MORGAN: I went to Oak Ridge in 1943, in September. The five of us went there. I left Chicago, of course, taking my family first back to Hickory. I was very cruel: I left my wife with three kids to do the packing and moving, and I caught the train and went to Oak Ridge. You must keep in mind that Hitler was winning the war and time was of the essence to get on with this research. It was after crossing over the [Solway River] Bridge on the bus from Knoxville that I went through the routine of being checked by guards, and then [living] some of our family history [and atomic history].

When I reached the staging area, you might call it—what now is called Oak Ridge—I was assigned a place where I would sleep and told the number of the bus that I would catch to go where I was to work. I was repeatedly cautioned about security and not to wander off the marked roads. A lot of the security, of course, was ridiculous and absurd, because it gave away what we were doing, for one thing. Any physicists would have to be very stupid not to know what they were doing at what was called Y-12 and operated, at that time, by Tennessee Eastman [Company]. You could see all the [large] transformers and power lines going in. There wouldn't be anything else in the world that it could be used for except an electromagnetic separation [plant]. Of course, what would you be separating? They wouldn't be making heavy water there; they were separating ^{235}U from ^{238}U and -234 [as I've explained in *Angry Genie*].

YUFFEE: Were you at X-10?

MORGAN: I was assigned to X-10. At that time, or shortly after, they called it Clinton Laboratories. Some years later it was dedicated as Oak Ridge National Laboratory, which is the present name.

CAPUTO: Why did you decide to go to Oak Ridge from the University of Chicago?

MORGAN: Well, at Chicago, living through part of the winter and a terrible [hot] summer there, [I couldn't wait to get to the cooler, open country]. I still felt like I was a Southerner and wanted to get back to the beautiful South and the mountains. I was working in my laboratory one afternoon when Martin Whittaker came in. He was a former classmate of mine at [University of North] Carolina when I was working on my master's [degree]. I used to help him with some of his mathematical problems that were pretty involved there at Carolina. So, I knew him very well. He startled

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15 white or colorless, tasteless, odorless, waxy, solid mixtures of alkanes, used especially in candles and sealing materials

me by saying, "Karl, how would you like to go south and get a job in the South?"

Well, I almost jumped to the ceiling [with joy] because I was very anxious, I and my family, to get back South. He said that he had been asked by Compton and by others, (I guess by [Leo] Szilard[16]), to head up a project they were developing in the cornfields of Tennessee, not far from a little town called Clinton. Whittaker said he would like me to join him in that program.

Creating a Health Physics Division (1943–44)

YUFFEE: Did you assume the role of director of the Health Physics Division upon your arrival at Oak Ridge?

MORGAN: When we got to Oak Ridge, Ernie Wollan immediately went into physics and left health physics. Ernie, unfortunately as you know, has long been deceased. The [1994] Nobel prize in Physics was given to one of his students there who he educated and trained in neutron diffraction techniques. Had he lived and were he alive today, he would be the principal recipient of that Nobel prize in Physics. Of course, that has been acknowledged.

Parker, Gamerstfelder, Hart, and I then constituted the original group [of health physicists] at Oak Ridge. Today there are over 35,000 professional health physicists in the world. There at Oak Ridge, Parker was the senior person in group, having been at Chicago prior to my coming. He left [Oak Ridge] later on in 1944. He, Gamertsfelder, and Hart left Oak Ridge and went to Hanford [in Washington State] in late spring and fall of 1944. Then the reins fell on my shoulders to do what I could to see that the pile we had in operation there was operated safely, and that the chemical and other operations were done without risk to employees or members of the public.

Concern for the Radiological Safety of Workers and the Nearby Public

CAPUTO: Do you know what drove that concern for workers' safety and the safety of the surrounding population?

MORGAN: Well, as I indicated earlier, we knew of only two risks. We supposed there might be others, but we knew nothing about them. The two were referred to as the "radiation syndrome," where doses of hundreds or thousands of roentgens[17] are received and cause skin erythema. I'll try

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16 Dr. Leo Szilard (1898–1964) was a Hungarian-born American physicist who with Walter Zinn proved the possibility of self-sustaining nuclear fission in1939 at Columbia University. At the University of Chicago Metallurgical Laboratory with Enrico Fermi, Szilard determined the amount, configuration, and means to control uranium fuel and directed the first nuclear chain reaction, December 2, 1942. He remained at the Metallurgical Lab until 1946, when he returned to his university position and concentrated on research in molecular biology.

17 a unit of radiation dosage equal to the amount of ionizing radiation required to produce one electrostatic unit of charge of either sign per cubic centimeter of air

to use the roentgen unit to be more or less consistent; otherwise I might use five or six others that are in current use. We knew that very large doses of ionizing radiation would be fatal to animals, and presumably to man. [The] thing we knew and were concerned about was skin erythema, which I've discussed. So, our main problem there was to make sure what the radiation levels were. When I say radiation, I mean energy above 15 electron-volts. Our main purpose was to know what the levels of dose were and to provide means of limiting exposure of workers and members of the public to what we thought would be a safe level.

CAPUTO: I was wondering about the concern for worker safety. Was that a legal concern, or was that just a general concern for welfare, or was it driven by public perception?

MORGAN: I can only state with certainty my own impressions and response. I'm sure that people like my friend John Wheeler and Arthur Compton and others at that [higher] level were concerned, not only about the science and engineering, but about the legal problems of exposure. Frankly, I was still a physicist at heart, not a health physicist. My only concern was obtaining knowledge and protecting people. I never at that time—we are now talking about 1943—considered the problem of legal restraints or litigation. Court and lawsuits never crossed my mind; I never even thought of it. To me, the important thing was to get rid of that bastard in Germany, to win the war, and protect the people.

Participation in Human Erythema Dose Studies, Using Phosphorus-32 (1943–44)

YUFFEE: I think, now, maybe we'll try to pick your brain to discuss some of the people who were performing research using human subjects. So I'll start off with a general question. What can you tell us about research that was performed at Oak Ridge using human subjects? At least, during the first part of your tenure there?

MORGAN: The only study using human subjects which I knew anything about, at the time, (this was 1943 and '44), was with respect to a small study in which I was involved, using phosphorus-32 plaques[18] to determine the erythema dose. Would you like me to tell you something about that study?

CAPUTO: Sure.

MORGAN: Well, as we have just discussed, at that time erythema was the principal concern, and we made darn sure no one would get a lethal dose. As soon as we arrived at Clinton Laboratories, we saw the workmen handling these little cylinders, called slugs of uranium, with their bare hands. Stone, Parker, Gamerstfelder, and I tried to get them to wear leaded gloves, but as soon as we turned our backs, they would be handling them with their bare hands. The uranium slugs were black and harmless-looking—just cold metal, as far as the workmen were concerned—and

[18] thin, flat plates with a thin layer of phosphorus-32

the lead gloves were very cumbersome and difficult to wear. They weighed a great deal and were not comfortable, so we felt we had a problem.

Keep in mind that skin erythema was the main concern. So, three or four of us got together and decided we've got to get some data, because there is nothing in the literature. We had already checked that thoroughly, of course. When I say "thoroughly," I mean not just the data from this country, but from the world bank of library information.

Well, it occurred to us [that] the best source to use, (and what seemed safest and most appropriate), was phosphorus-32. We knew from studies from people like John Wheeler and others, that sulfur-32, if exposed to fast neutrons above 5 million electron-volts [(MeV)], knocks out a proton, and the neutron sits in its place in the nucleus. So the atomic mass doesn't change, and the atomic number then would go down by one. This changes sulfur-32 to phosphorus-32.

We got some sulfur cylinders and had them sawed so we had little plaques about an inch and one-half in diameter and a quarter of an inch thick. We then put them in the reactor, in the fast neutron area near the slugs of reactor fuel, and "cooked" them, as we called it: allowed them to be exposed to fast neutrons for an appropriate period. This process, in which the fast neutrons above 5 MeV were replacing protons in the nuclei, converted some of the sulfur into phosphorus. Then we had little plaques that were very radioactive, that had a considerable amount of phosphorus-32, a strong beta emitter. Of course, this produces bremsstrahlung[19] and x rays, as well. We took these little plaques, and taped them on our arms and legs, in some cases, and [on] about twelve girls [(young women)] we called "meter readers," and nurses.

CAPUTO: So you were a subject in this experiment?

MORGAN: Yes, I took part in the experiment. We kept them on our bodies in this manner for some time. After about a week, some of the blonde girls, in our [group of] meter readers, and some of the nurses, began to complain that their arms were itching and painful. So we lifted up the plaques a little bit and looked under them, and sure enough, the arms were very red, and we watched carefully.

Shortly after that, [our medical] doctors reported seeing some signs of early ulceration from the white pimples beginning to show up among the red patches. So we got quite worried, and of course immediately removed the [plaques] from the young ladies. Of course, older people, like Stone, Parker, and myself, who were used to some hard work, had a thicker epidermis.[20] The young blondes, they were, especially, more susceptible than the browns and brunettes.

YUFFEE: Because of their fair skin, you think?

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19 radiation, especially braking radiation, gamma rays, or x rays, emitted by decelerating charged particles

20 the outermost layer of skin

MORGAN: The thickness of the epidermis. As you know, the erythema begins primarily in the epidermal region. So, we carried on an experiment that I would never dream of doing today, or would not have dreamed of a few years later at Oak Ridge when I became HP [(Health Physics)] director. I wouldn't have done it except [after] having done a great many preliminary studies, which we did *not* do. I would have rushed to have put it first as a very rushed project, one that you had to pursue with all speed ahead, because we had the workers that were potentially in trouble, but we were sort of working in the dark and taking big risks, which we should not have taken, without knowing better what we were doing.

You can think, perhaps, of better studies. For example, we could have put these plaques on some rabbits and dogs and maybe even on some primates, monkeys and baboons, before going to *homo sapiens*.[21] I think I'm guilty. The other parties, except for Gamertsfelder, are all deceased, so I'll take the blame.

We found experimentally the human skin erythema beta-particles dose, and that was the dose from the surface of uranium. Parker and I independently later did theoretical calculations and [determined] that the beta dose would be about two hundred and sixty mr [(millirem)] per hour. I carried on further theoretical studies and found the x-ray and alpha dose, but they were not important in this case. This is the first human study of three that I or my group, the Health Physics Group, conducted at the Laboratory.

CAPUTO: Do you remember the decisionmaking process? How that experiment got approved, or what steps you had to go through before you could conduct the experiment?

MORGAN: At that time, you have to remember that we were primarily scientists with no supervision. Scientists don't like to get approval from anybody. They always like to feel they know their business, they know what they're doing. They don't bother their boss; they don't feel they *have* a boss! Good scientists don't. I guess we were a bit arrogant and impudent maybe, in some respects, but that's the way scientists were; and I think, for most part, top scientists are that way today. They don't like to feel they have to go through the boss and say, "Can I turn this crank this direction so far?" or "What will the electrons do in this case?" So we asked nobody, and got permission from nobody, and nobody expected us to get permission as we determined the human erythema dose.

Human Research Protocols; Informed Consent

CAPUTO: Did you write a protocol before performing the experiment or talk among yourselves about how it would be conducted?

MORGAN: We, of course—as we had lunch at the cafeteria, and maybe a beer in the evening—discussed various ways to get the needed information. At that

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[21] "man"—the species whose members are known popularly as "human beings"

time in Oak Ridge—you remember it wasn't called Oak Ridge, just a little community near Clinton—we got together sometimes and discussed our work in the lab lunchrooms, but we were very careful not to even suggest anything about radiation except at the laboratory. So far as I can recall, our only discussions were the physics and mechanics of the problem of making ^{32}P [(phosphorus-32)] sources, and we took for granted that no one would object to us getting this sorely needed information when we would cause them no harm. We thought that the skin erythema would be just a little bit of sunburn-equivalent on your arm, and nobody worried about *that*. Even then, people sat out in the sun to improve their complexion. There was no problem, and we were not expected to [have to] discuss this with management or anyone else.

YUFFEE: Outside of the Health Physics Division, were you aware at that time of any biological research that was going on, say, with the Biology Division?

MORGAN: I can say categorically that as far as the Biology Division is concerned, though I was not intimately aware of many things that were going on there, I knew Alex Hollaender[22] well enough to believe that he would have opposed any potentially harmful human studies with the same vigor that I did and would have done [were I head of that division]. I would have to see, in indelible print, evidence that he or anyone in his group did human studies or used humans as guinea pigs, (to put it more crudely.) Things could have happened of which I was not aware of, since a big portion of the time Alex was working in the Y-12 area, which was about six or eight miles separate from the X-10 area, where my laboratory offices were.

CAPUTO: I was curious: you weren't really allowed to talk about radiation outside. What were the workers told about the risk of radiation, the ones who were handling the material in that early time period?

MORGAN: In that early period, they were only told, "You don't want to expose your hands and face to it or you'd get erythema." That's all we knew.

YUFFEE: Were they told what erythema was?

MORGAN: "Sunburn," I guess we used that instead of the word "erythema"; but with the mechanics and meter girls and so on, if we said "erythema" we might have [given the] Greek or Latin [origin of the word erythema] and might have gone into that a little bit, explaining the red injury.

Plutonium Injection Studies at an Oak Ridge Military Hospital (1945)

YUFFEE: I guess I have a specific question about one of the more infamous studies that took place that we know about: the plutonium injections. One of which we know was given at the hospital in Oak Ridge in, I guess, April of 1945. Is that something that you knew of back then?

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[22] director of Oak Ridge National Laboratory's Biology Division

MORGAN: Yes.

YUFFEE: You knew that the injection was going to take place prior to it?

MORGAN: No.

YUFFEE: Do you know who performed the injections?

MORGAN: No. Do you want me to tell you what I know about it?

CAPUTO: Sure.

MORGAN: Bob Stone—the associate director [for Health] under Compton—had his office next to mine at X-10. One morning, he came in all excited and upset. You will have to put this in context of the time and the location that we were in. We were in the South, and it's no reflection on the African Americans, but they were called "niggers." I'm only telling you as I recall; my memory is far from perfect.

As I recall, he said, "Karl, you remember that nigger truck driver that had this accident sometime ago?" I said "Yes," I knew about it. He said, "Well, he was rushed to the military hospital in Oak Ridge and he had multiple fractures. Almost all of his bones were broken, and we were surprised he was alive when he got to the hospital; we did not expect him to be alive the next morning. So this was an opportunity we've been waiting for. We gave him large doses by injection of plutonium-239."

Of course, when you say "-239," it has some [plutonium]-238 and -240 mixed in, but [it is] primarily -239. [For security reasons, the word "plutonium" was never used in 1943–44. Stone continued,] "We were anticipating collecting not just the urine and feces but a number of tissues, such as the skeleton, the liver, and other organs of the body. But this morning, when the nurse went in his room, he was gone. We have no idea what happened, where he is, but we've lost the valuable data that we were expected to get."

I had not even heard of the experiment. I learned later that Stafford Warren[23] and Hymer Friedell[24] and the others apparently knew about the study, but my project was primarily with physics not with medical or biological studies. So this was the first I heard of the situation.

I heard nothing more about this till some years later. I happened to see a little notice in the Knoxville paper, the *News-Sentinel,* stating that this man, "a black man"—our society had evolved a little more at that time—had died someplace in eastern North Carolina, as I recall they must have given enough information that I could tie it in with the same fellow.

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23 a professor of Radiology at the University of Rochester, Rochester, New York, site of research involving plutonium and human subjects. Dr. Warren worked on the Manhattan Project and headed an Intramedical Advisory Committee.

24 For the transcript of the interview with Friedell, see DOE/EH-0466, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Hymer L. Friedell, Ph.D.* (scheduled to be published later in 1995).

Then I heard nothing about it till recently. Only recently, more recently in the past few week, I have heard the name of the fellow and more information about his family, etc.

CAPUTO: Who would have had the authority to provide the plutonium for the experiment?

MORGAN: Who would have the authority? That's a good question. In spite of our security, in some ways it was provided in a very ridiculous manner. I think I could have gotten all the plutonium that could be provided for anything I wanted to do, if it could be spared. Joe Hamilton[25] got a dribbling amount to supplement his studies that he had done with plutonium-238, [which] he had gotten from the accelerator. I'm sure, confident, that if I'd put the request in, I could have gotten it. But, I suppose, all I would have had to do is walk in Martin Whittaker's office and say, "Martin, we want to do this experiment. We need so many, two or three microcuries." He would have provided it.

CAPUTO: So Martin Whittaker decided, since there was such a little amount of plutonium [available at that time], what had priority—

MORGAN: —In that period, it was very informal. We knew that we had to follow very stringent restrictions to prevent useful information from getting out. You have to keep in mind that during the first several months—this was in the early period [of the Manhattan Project]—health physicists, seniors [like me], were primarily physicists, and the doctors and surgeons were primarily doctors and surgeons, not people working with plutonium. So with all of us, we did the best we knew how, and I think we did a tremendously good job considering our background, and what we were trying to do, and what our major job was.

I don't think it would be any problem in getting the plutonium. Probably— my guess would be that Hymer Friedel or Stafford [Warren] were brought intimately into the earlier stages of [this study]. I say that without any great knowledge, but only because I knew both parties quite well at the time and knew what their interest were and what one of their main goals was: to get information on the risks of plutonium [and uranium]. Was it *as* hazardous as radium or *more* hazardous, [was] the essential question.

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[25] Joseph Hamilton, an M.D., worked at Crocker Laboratory, then the site of a 60-inch cyclotron that he operated to produce radioisotopes in support of research and some medical diagnosis and treatment. Crocker was part of the Lawrence Radiation Laboratory, later renamed Lawrence Berkeley Laboratory, located at the University of California at Berkeley. Hamilton is discussed in several transcripts of this series, notably in the interviews with John Gofman (DOE/EH-0457, June 1995) and Earl Miller (DOE/EH-0474, June 1995). Hamilton spent most of his career at the Laboratory before dying prematurely of leukemia brought on, colleagues believe, by occupational exposure to radiation.

Oak Ridge Committees (Isotope Distribution, Human Use, et al.)

YUFFEE: Moving out of the postwar era, were you familiar with Paul Aebersold[26] and the beginning of the Isotope Distribution Committee?

MORGAN: Yes; I knew him and his wife Mickie quite well: They were in two [of] our dance clubs. Mickie and Paul were excellent dancers. In fact, they gave exhibitions, taught us a lot of the Latin American dances.

YUFFEE: Do you know who else was on the Isotope Distribution Committee? Were you on the committee itself?

MORGAN: I don't remember if I ever knew; I don't think I ever knew.

YUFFEE: Okay.

MORGAN: I probably knew all the people on the committee, but I knew nothing *about* the committee. I tried to stay away from all politics, all public relations, union problems. Those things I tried to avoid as much as I could.

CAPUTO: Do you know what year Oak Ridge National Laboratory began [its] Human Use Subcommittee?

MORGAN: No, I don't.

CAPUTO: No. Were you on that subcommittee once it started?

MORGAN: I could have been: I was on thousands of committees. Many of them never even met, so I don't recall having been on that specific committee.

YUFFEE: Were you on any committees that might have determined safe levels of exposure to radiation for either employees or for subjects and further studies, further research?

MORGAN: If you knew me, that would be sort of a ridiculous question, because I was on hundreds and hundreds of such committees at the time. I was at Oak Ridge for 29 years, so if you want to go down the line, I could. I was on many, many committees that dealt with those problems of permissible exposure. I guess the majority of them had to do with internal exposure.

You probably know—I'm sure you do—that I published probably the first paper ever published on suggesting or indicating mathematically how to calculate the permissible dose of radioactive material, once into the body, taken from food or air or water. I've said, on the basis of those calculations, comparing primarily with radium and with an arbitrarily selected level of 15 roentgen per year.

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[26] Dr. Paul Aebersold established the administrative system for distribution of radioactive isotopes. After working on the Manhattan Project at Los Alamos, New Mexico, and Oak Ridge, Tennessee, from 1942 to 1946, he served as director of the Atomic Energy Commission's Isotopes Division at Oak Ridge from 1947 to 1957. He retired as the Director of the AEC's Office of Isotopes Development in 1965. Two-and-a-half years later, he committed suicide. For additional information on Dr. Aebersold, see "Safety of the Nuclear Industry" in the interview with Merril Eisenbud (DOE/EH-0456, May 1995) and "Remembrances of Personalities" in the interview with Earl Miller (DOE/EH-0474, June 1995).

Based on that background, having set the levels at the Laboratory and the levels that were rather automatically adopted at Argonne and Hanford, I was asked to be chairman of the Internal Dose Committees of ICRP[27] and NCRP,[28] the job or position I held for about twenty years. In a very early period, I followed through the same procedure of calculations of permissible levels for all the major radionuclides[29] that we were concerned with in the early period. It finally got out of Security and was published in the *Journal of Organic Chemistry*.[30] You probably have a copy. I have it, but not here at my home in Florida.

Studies in Uranium Ingestion, Injection, and Inhalation

YUFFEE: In reviewing some of your bibliographic information on your publications and such, and also reviewing other documents, we've noted that you did some work with Robert Bernard in his uranium research. I was wondering if you can tell us a little bit about that.

MORGAN: You mentioned something about the work of Bernard. As I indicated earlier, we began with actually four members of the Health Physics Division of what became Oak Ridge National Laboratory in time. Shortly after our arrival there in 1944, I became director of the Health Physics—what was later called Health Physics Division. By the time I left Oak Ridge at retirement, we had over 200 [people] in the Health Physics Division. Somewhat less than half of those were [working] in the Applied Health Physics [Group]. That is, they made measurements of the activity at White Oak Lake and the rivers and in the air, and operated the meter program and so on. [The others were engaged in research].

The[y] were [engaged in] various branches of research. One type of research had to with getting better values on internal dose of the radioactive materials. In fact, that was the principal research I eventually engaged in along with Dr. Walter Synder, my assistant director.

Dr. Bernard was head of the one of the [Health Physics] research groups that did research in this area. In the course of this assignment, he and his group, as I recall, on several occasions, agreed to use themselves as guinea pigs. They ingested ^{131}I. I don't recall any inhalations or injection

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27 International Commission on Radiological Protection

28 National Council on Radiation Protection. Although the words "and Measurements" were later appended to the name, the council's initials remain NCRP.

29 radioactive nuclides (atomic species in which the atoms all have the same atomic number and mass number)

30 After the interview, Morgan submitted the following clarification: "Insofar as I can determine, I published the first paper in the open literature showing how to calculate permissible levels of exposure to radionuclides (Morgan, K.Z., 'Tolerance Concentration of Radioactive Substances,' *J. Physical & Colloid Chemistry, 51*, p. 984). All my previous publications were on cosmic radiation and on the meson, the fourth basic particle of matter. Now there were known four basic particles: the electron, the proton, the neutron, and the meson. All were published in the *Physical Review* (Vol. 52, No. 6, Sept. 1937; Vol. 54., No. 4, Aug. 15, 1938; Vol. 56, No. 11, June 1939; and, Vol. 57, No. 2, Jan. 15, 1940). These publications were [written] jointly with W.M. Nielsen and L.W. Nordheim of Duke University."

studies, though they might have had a few of several of the radionuclides: I'd hate now try to recall which [radionuclides they ingested]. I do think maybe strontium-90 could have been one. I won't even try to remember the others.

But one of our big [occupational health hazard] unknowns, along with plutonium, was of course that of uranium, because we were not just working with the quantity, we measured in microcuries, but in *tons* of uranium. So from the neuron and urine and fecal analysis program in health physics that was developed initially by—Ralph Firmanack was one of them; I'll think of the other names perhaps in a little bit—we developed several techniques for the assay[31] of these uranic and transuranic[32] elements.[33] We were at great difficulty in assigning a permissible level for uranium.

I indicated earlier, by this time—this was after 1950—I became a member of ICRP and NCRP. ICRP, you recall, was rejuvenated about 1950. Prior to that and prior to the war, there had been an organization which claims to be the precursor of ICRP, and it's okay to call it that, but it was really an entirely different body and a different structure. ICRP, as-is and as-was, really began in 1950 when I joined; and I was a member, as I indicated, [of] the main commission of 13 members, for about twenty years. I was chairman of Internal Dose of that [international] group and [of] the national group—NCRP—for that same period.

We had sort of an anomaly with uranium: it was the only radionuclide where it appeared that the *chemical* risk was equal or even greater than the *radiation* risk. Now, in the case of plutonium or strontium or cesium or americium—any of these others—it was almost absurd to think in terms of [units of mass as small as] grams or milligrams. But in the case of uranium, that was not the case. The data seemed to indicate that uranium as a metal was very toxic. It went to kidneys and some to the liver, a little bit to the bone, but mostly to the kidneys, and it was quite toxic. A lot of this early work, of course, was done at the University of Rochester [in Rochester, New York] and some of the hospitals in that city.

YUFFEE: Those people—would that be the uranium injections that you are referring to?

MORGAN: They not only did injections, but studies of inhalations and ingestion, as I recall. I don't recall at which institutions. A good bit of it was at [various hospitals] located there [in the Rochester area], of course.

Stafford Warren was sort of a fine, but blustering scientist as well as a military officer. Sometimes, when he would be in a scientific discussion, he would sort of forget—he'd think he was still an officer in the mili-

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31 determining of the amount of material present in tissue, urine or feces by any trial measurement

32 having an atomic number higher than 92, the atomic number of uranium

33 Morgan adds: "Our first studies on body fluid analysis were conducted by Ralph Firmanack and Larry Farabee. They developed the early methods of determining uranium, plutonium, and strontium (^{238}U, ^{239}Pu, and ^{89}Sr and ^{90}Sr) in urinal feces, among other research."

tary. So friends of his think of the whole Warren when we think of him, not just as you read about him or as you may see him in a certain context.

Struxness and Bernard Go to a Boston Hospital to Assist in Studies in Radioisotope Injection Toxicity (Mid-'50s)

MORGAN: [In any event,] Bernard was quite interested and anxious to get this data. In some publications he read that some work was going on in Boston, and he got in touch with that group. They invited him to come up and work with them.

YUFFEE: Were they at MIT?[34]

MORGAN: No, I think it was at Boston General Hospital, as I recall. You have to remember, I'm recalling things that I've not even bothered to think about in well over 50 years. I never write things down. The memory pad I have above my ears up here *(points to his head)*, sometimes I think it's getting a little bit illegible. Anyway, as I recall, these reports that they [(Bernard and Struxness)] came across, particularly Bernard—and you keep in mind, we had to be very careful how we published things in those days. They were voluminous, reports that came into our attention at the Laboratory describing what was being done under the auspices or funding of what was then called the Atomic Energy Commission.[35]

In the health physics research organization under my direction and then under the immediate direction was Dr. Struxness, Edward Struxness. He was likewise quite concerned, so Ed Struxness and Bernard went to Boston and participated in this study. They were welcomed, in particular, because they knew dosimetry, and the group there had just started blundering their way along administering a metal. [The Boston team] could measure grams of metal—micrograms, maybe milligrams—but we did things in terms of microcuries—that is, a millionth of a curie. We had good techniques for measuring alpha radiation, and as I recall from the reports there [at the Boston hospital], [their] instrumentation was relatively crude compared to what we had at that time developed at the Laboratory.

So, Struxness and Bernard went there and worked with them awhile, and shortly afterwards Struxness came into my office and said they had quit. I said, "Why are you back? I thought you [were] to be in Boston. You were supposed to be there for a month or so." He said, "Well, they were doing things that were very irregular and improper and was harming patients, causing them extra pain and early death," and he wanted nothing more to do with the study. I won't say under oath that this is verbatim what he said, but it's as close as I remember. This is certainly the

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34 Massachusetts Institute of Technology, Cambridge, Massachusetts

35 The Atomic Energy Commission (AEC) was created in 1947. Morgan meant to say the Manhattan Engineer District (MED).

context of what he was saying to me as best I can express it many years later.

Ed never went back to this program, and as far as I know, our cooperation with that program was [temporarily] terminated.

Now, when you have a program of maybe twenty different programs going on all along; then, in addition, applied health physics, I as a [division] director did not necessarily follow all the details. So I don't know how long Bernard stayed there after Struxness came back. I would guess he probably came back to Oak Ridge about the time Struxness did. But I don't know what [Struxness and Bernard] may have told them there, whether it was in a few [harsh] words or what. Ed Struxness is still living and I expect that Bernard is; I don't know. [I suggest you contact them].

YUFFEE: He is.

MORGAN: Is he? I'm glad to know that. He's much younger than I, but Ed is in rather poor health.

YUFFEE: Do you remember in particular what the study up in Boston involved? In terms of: If Struxness said they were harming the patients, did he go into specifics?

MORGAN: The best way to answer your question would be to get the early report. They give the microcuries and micrograms, and the chemical forms that they used in, these injections. Rather than try to recall the chemical forms and the quantities, I suggest you refer to the published reports, or the written [internal] reports. I don't know if—most of them were not published in open literature, but I'm sure that the reports are available; some at the Laboratory [(ORNL)] and some at the hospital.

CAPUTO: Would the isotopes for that experiment [have] come from Oak Ridge?

MORGAN: I would guess, though again I'm not sure. My guess is based on a few tidbits of memory. I think that some of the studies were with enriched uranium; maybe they used different enrichments, but I can't be sure. I seem to vaguely recall that they were [using] some enrichment of the ^{235}U. Whether they tried to get studies where the gram enrichment of -234 was enlarged, I don't know. That would be, of course, much more hazardous, radioactively. These quantities would have come at that time [from Y-12 or K-25 or both]—what was the year? Do you have the approximate year?

YUFFEE: I thought it was in the mid-'50s—'55, '56.

MORGAN: So, the K-25 [gaseous diffusion plant at Oak Ridge] would have been in full operation. So it [(^{235}U)] would have come by [way of] K-25—the electromagnetic process [had] long-since given out at Y-12 and Tennessee Eastman had long since left Oak Ridge. So it would have originated—most likely—from K-25, though it could have [come] from Paducah[, Kentucky] or one of the other places.

YUFFEE: But it definitely would have been from the Oak Ridge area?

MORGAN: No, it could have been Paducah or one of the other uranium separation plants. Such as I recall, some of it was enriched [in ^{235}U].

Criticizes Therapy Practiced at ORNL's Total-Body Irradiation Facilities

YUFFEE: In terms of other researchers, could you tell us if you know anything about other research activities at Oak Ridge, maybe by people such as Gould Andrews and Marshall Brucer?

MORGAN: Marshall has recently deceased [(1994)] and Gould [has been deceased] for a long time. I think the principal culprit is still alive, Clarence Lushbaugh.[36]

CAPUTO: We've spoken to him.

MORGAN: He was a former friend of mine. I don't hate him, but I don't like what he did. My religion tells me not to hate anyone. What would you like to know about it?

YUFFEE: Do you know of any specific research activities of Gould Andrews, for example?

MORGAN: I knew Gould Andrews quite well. We belonged to some of the same committees, and I was with him overseas on several occasions. Of course, in Oak Ridge, we helped with the dosimetry there (at ORINS).[37] We had nothing to do with the instigation and planning of any of their studies. We did help them somewhat—or quite a bit, I guess—in some of the calculations, because I had some of the best physicists in the world in my group. We helped them with dosimetry with the instruments, so that they could get much more precise data relating the syndrome to the radiation dose.

During that period—this is not long before I left Oak Ridge, before I retired in 1972, September—after 1955—it's quite a bit after that, probably was in the '60s—I visited there at the invitation of Gould. I visited the facility and saw what they called the "radiation rooms." I call them "radiation chambers" now, thinking of what went on under the human devil named Hitler. There I saw how they raised sources to the ceiling and the walls and the floor. I was quite satisfied with the safety and the arrangement (as far as the workers were concerned) and how to get [the radiation] back in the containers, pneumatically.[38] I was satisfied with

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36 For the transcript of the interview with Lushbaugh, see DOE/EH-0453, *Human Radiation Studies: Remembering the Early Years; Oral History of Pathologist Clarence Lushbaugh, M.D.* (April 1995).

37 Oak Ridge Institute of Nuclear Studies, established in 1946 by the Manhattan Engineer District and operated under a Manhattan Project (and later Atomic Energy Commission) contract. ORINS was responsible for training physicians and researchers in the safe handling of radioisotopes and in the development of isotope applications in medicine. In addition, ORINS was responsible for selecting both students and established scientists for fellowships and other temporary research assignments. Today, the educational and training functions of ORINS are carried out by its successor, Oak Ridge Institute for Science and Education (ORISE).

38 using pressurized air

the instrumentation of estimating the dose at the various chairs and cots in the two rooms.

Then, later on, my former Sunday school teacher[, Jim Fisher from Salisbury, North Carolina,] was one of the patients there. He came, and he looked to be in rather vigorous health [when he entered]. I was amazed at his demise [once treatment was underway]. Shortly afterwards, he died. I was very naïve. I guess I was too concerned looking after the research of well over 100 people and trying to protect the people from our operations at Oak Ridge to question, in any detail, the program they were doing [at Andrews' facility].

CAPUTO: Which program was this? Was this the METBI?[39]

MORGAN: I don't recall the names of the other people, for certain. I could guess at them, but I'd rather not. I know Marshall Brucer was there. I know, of course, because Marshall and Gould were rather close friends of mine at the time. I met the other doctors there, and I have heard of them since, but since I don't keep anything in writing, it's only in my head, I don't recall any names of other doctors [doing their treatments].

YUFFEE: This would be the total-body irradiation studies?

MORGAN: There were two rooms.

CAPUTO: The medium-exposure [irradiator room].

MORGAN: Right; they had code names for them and I could perhaps recall them, but I won't bother since I'm sure you know them. If I were in my office, at a thumb's distance I could put pick up a pamphlet and tell you the names of the other people there, but it wouldn't be from memory; it would be only from what I read now.

Anyway, I was familiar with the fact that they had these exposure areas. I was very interested that my Sunday school teacher was there, whom I had the highest regard for. But I was very depressed that his [disease] seemed to progress much more rapidly than I would have expected from my meager reading of the literature.

Most of the cases [at the facility] were leukemia—I think primarily three types of leukemia. At that time, I and my group—and that included particularly Sam Hurst and Rufus Richey, and later a few others tried to evaluate all radiation accidents. Had there been an accident or were there an accident anywhere in the world, my phone would ring shortly afterwards. Usually I would be urged to catch the plane and get there immediately. Such as Windscale[40] and the accident in Yugoslavia, and

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39 Medium-Exposure-Rate Total Body Irradiator

40 The first British production reactors went into operation in 1950–51 in Windscale, England on the Irish Sea. In October 1957, an incident occurred at Reactor Number One which resulted in the release of excessive amounts of radioiodine and other radioisotopes to the environment. Use of milk from local farms was discovered to pose the greatest radiological health hazard to the local community.

the accident at Idaho Falls, and so forth. "SL-1" they called it [(the Idaho Falls accident)].[41]

Here in Oak Ridge, we had this facility, of which I was quite proud, and thought that they were using the kind of [unorthodox] treatment, for example, [that had been] applied [in Paris] to the young lady who was the first victim that I knew of out of the country, in Yugoslavia. You remember, she was sitting there at the control panel of the reactor. Everything had gone so well, and it's very boring to sit there with nothing to do. She was studying English, of all things; trying to improve on her English. This darned old Geiger counter on the wall was clicking away, and that day it was particularly worrisome, and it had been doing [this clicking] for quite a while.

So[, convinced the counter was malfunctioning], they went about their work, paying no attention to counter, when suddenly a health physicist—I forget his name now—came in the door. Fortunately, as all good health physicists do, he had his Geiger counter turned on as he walked through the facility. As he approached the room, his Geiger counter went wild, and when he opened the door it was clicking like mad—almost blocked out. Of course, then everybody in the room ran out, because they knew that *their* Geiger counter wasn't fooling—that *they* were the fools.

None of the others—I've never heard or tried to follow up on what happened to them, only this one young lady. Our group did determine the dose they received. [We made our determination] from the activation of sodium-24 in the blood and sulfur activation, producing, as we discussed earlier, the phosphorus in the hair: from getting hair samples, [taken from] different parts of the body, you can determine the distribution, geographical distribution of the dose [throughout the body], and so forth.

But this woman was treated locally there, at the [Yugoslav] hospital, but with no success whatever, and moved to Paris, where Drs. Bugnard and Jamet were the two principal doctors on the case. There was another [doctor]; I can't remember his name. They treated this patient with what they called the "push-pull" method—that is, where the [bone marrow] donor and the patient lay side-by-side on cots. They push the needle into the sternum[42] of the donor and immediately into the patient that had been exposed. As with all exotic treatments, she deceased. I don't know whether it's proper to say that the person died or not. One doesn't like to think there is such a thing, I guess.

Anyway, I was aware of all of this. I was aware of the treatments that had been proposed [by some at Oak Ridge] and then studied on the

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41 The Idaho Falls National Laboratory accident, SL-1, was a reactor accident that resulted in the death of three workers. For an extended discussion of the SL-1 reactor accident, see "Fatal Worker Accident at Idaho's SL-1 Reactor (1961)" in DOE/EH-0454, *Remembering the Early Years: Interview With Dr. George Voelz, M.D.* (May 1995).

42 breastbone

animals, and the failures that we'd had, especially this particular failure case [of my former teacher]. I suppose I just took for granted, since Gould was director of that lab,[43] that his subordinates there would follow all these same procedures. Of course, we knew that if you destroy the active bone marrow, sometimes called red marrow, in a patient, the reticuloendothelial system[44] no longer exists—the immune system's gone—and you have to be very meticulous not to allow a single germ to get anywhere near these patients. You had to follow it with a protective regimen.

Some regimens seem to hold promise for animals. A few cases [show] very encouraging promise on animals. But, at that time, I knew of no real studies that made me jump up and down with enthusiasm for studies on humans. For the most part, the studies had been very discouraging using radiation. So, I did not question it entirely because of my respect [for] and knowledge of Gould and what I felt was his ethical standard.[45] Then, after I left Oak Ridge and became a Professor at Georgia Tech, I was appalled to read in the news and *Mother Jones Journal*,[46] I believe was the name of one of the journals, about a little boy who was brought there with high hopes of the mother and fairly good status at the time, though he had a fairly advanced case of leukemia—I don't remember, was it myelogenous?[47]

CAPUTO: That's the Sexton case.

YUFFEE: I think it was myelogenous.

MORGAN: Well, that, of course, is very definitely [a case of human experimentation] associated with radiation as one of the primary causes [of death], and so I would [consider this defective] treatment if the proper procedures hadn't been followed.

Then at that time, in *Mother Jones*, I read some of it and then I testified [on behalf of the little boy] later on in Oak Ridge in the case before our Vice President.[48] We won the case scientifically, but [then-U.S. Representative Al Gore] decided the case politically, as most politicians are supposed to do. They're not supposed to help people but to help their jobs, I guess, to help politics. So that was the way it was decided.

It was very evident to me that there were irregularities, very serious irregularities in that case. I mentioned earlier that you should fight to

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43 the total-body irradiation facilities

44 a family of cells that function in the immune system's defense against foreign bodies

45 See also the comments by Lushbaugh in the section of his interview, "Charges That the Oak Ridge Radiation Therapy was Not Effective."

46 *Mother Jones* is a monthly magazine specializing in investigative journalism from a progressive political perspective. It is published by the Foundation for National Progress.

47 originating in the bone marrow

48 Hearing Before the Subcommittee on Investigations and Oversight of the Committee on Science and Technology, U.S. House of Representative, 97th Congress, First Session, September 23, 1981, No. 63: *Human Total Body Irradiation (TBI) Program at Oak Ridge,* U.S. Government Printing Office, Washington: 1982.

keep the last germ out of the facility, and when I testified under oath, [I stated that] I had learned that they were hauling animal droppings from mice, dogs, and rabbits through the same facility. I could not believe that such a thing went on! I heard this very often [and] that [the expected] follow-up treatments were not carried out.

Of course, I've heard about some other stories since then. More recently my wife and I [went to the door when] our doorbell rang. Mr. Litton and his wife came to our door in Oak Ridge. I don't know whether you've met him yet or not. He told of an awful case of his father, how he was treated there. He was in fairly good health when he went there, and they felt encouraged that it was so near where they lived. I think they lived down near Oliver Springs[, Tennessee,] at the time—a few miles out of the city limits of Oak Ridge. So they brought him there. They thought it might help his case, cause some recession of the pain he was having.

He died very shortly after coming there and [going downhill] physically, very rapidly. He [(the deceased patient's son)] got hold of some of the dose records and [discovered that] they went eventually up into the thousands of roentgen. (Maybe I'd better switch over to units of rads now, or sieverts,[49] if you prefer.) They went up into the thousands of these units. I was just appalled at what he told. It's a very pathetic case.

I heard of other cases, and I think the case of Clarence Lushbaugh's treatment of humans as guinea pigs and Eugene Saengers at the hospital in Cincinnati are some of the most terrible human studies I ever heard of other than those that took place in Germany, and a few in Japan I've heard [that took place] during the war. I was very disgusted when I read in the *Oak Ridger* some time back, a few months ago, about a woman—you mentioned her name earlier—that worked with Gould Andrews and with Lushbaugh.

Hidden Military Funding to Explore Radiological Warfare During the Cold War

CAPUTO: Ann Sipe?[50]

YUFFEE: Or the Sexton case, the mother of the boy.

MORGAN: Not necessarily, but there now, you said [you] interviewed her, I think.

CAPUTO: Helen Vodopick?[51]

MORGAN: Vodopick had a little article in the *Oak Ridger* stating that the monies they got from the Pentagon were all minuscule, very small. As you say,

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49 the Systême Internationale (SI) unit of dose equivalent when the absorbed dose is measured in gray

50 Sipe worked with Lushbaugh at Oak Ridge in the 1960s, serving as the day-to-day manager of the Low-Exposure-Rate Total Body Irradiator (LETBI). She was present during the Lushbaugh interview; her comments are found throughout that transcript (DOE/EH-0453). In that interview, Lushbaugh and Sipe vigorously challenge charges that their therapeutic radioisotope treatments were unethical.

51 For the transcript of the interview with Vodopick, see DOE/EH-0482, *Human Radiation Studies: Remembering the Early Years; Oral History of Dr. Helen Vodopick, M.D.* (August 1995).

to start with, you're interested in getting the facts, and I wouldn't talk to you a moment unless I took for granted that you are [interested in the facts]. If you're here only to protect, as has always been the case in the past, to protect the past actions of the DOE, you might as well leave and run.

CAPUTO: *(smiling)* We're "reinventing government."

MORGAN: If you're trying to get the facts, I would urge you to look at the [research] budget and find how much came from the Pentagon. She [(Vodopick)] said, according to this article, the amount they got from the military was insignificant. I think she put in her denominator,[52] "the cost of building the hospital." If I put infinity in the denominator of any equation, the answer is zero! So it approached zero, according to her. So, if you haven't [determined the amount of money the Pentagon put into this program], I would urge you to do that. If any good comes out my discussion here and you do that, I'll be proud of having talked with you. But I doubt if that has been, to the present moment. But if it was only a few pennies that the military put in it, let's forget it. But if they put as much as 10 percent or even 5 percent—[I believe the guilty person should be punished].

CAPUTO: I heard 10 percent as the figure.

MORGAN: Let's get the figures; lets see the numbers. I mean, this isn't some profound research like we did earlier at Oak Ridge! This is something that even I can do—and I don't have a degree in Business Administration. I don't understand—maybe it's been done and you just haven't come across the figures. You ought to know to the penny how much came from the Pentagon and how much from [Oak Ridge] Associated Universities.[53] Because that [figure] is being used to show that it was no problem.

The real problem is that during that whole period, I got [many] letters, and I got visits, from not just [from] the two Warrens—mostly from Stafford Warren, not so much Shields at the time—urging me to talk to any military officer, any official after showing appropriate identification, on the concurrent use of radioisotopes in warfare, along with various chemicals. I visited Dugway [Proving Grounds[54] in Utah] and I had discussions with many officers and people from Washington [regarding how enemy forces would react were they exposed to high doses of radiation from radionuclides (mostly fission products)].

Of course, I had numerous discussions with my friend Stafford Warren at the time. —I don't want to get the two Warren's mixed up: Shields is

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52 the portion of a fraction that appears below the "divided by" line—the amount into which the value above the line is divided

53 managing and operating contractor of the Oak Ridge Institute for Science and Education, formerly known as Oak Ridge Institute of Nuclear Studies (ORINS)

54 operated by the U.S. Army, Dugway Proving Grounds is the field test site for U.S. chemical warfare agents. Dugway also has been used for radiological effects testing.

a very wonderful person, with a high level of integrity and morals. I never had reason to question it of the other Warren, but he was a typical military type, [who believed] that military answers are *the* important thing.

You know, [it's] on the record that the [AEC's] directors of Health Physics and Biology and Medicine met about four to six times a year at one of the sites. So, [during] the 29 years [Iwas at ORNL], if you multiply that by four, you see I had over a hundred meetings with people, many of which both Warrens attended. People like Wright Langham at Los Alamos and people like, say, Hurst at Rochester, John Rhodes at Argonne, and later, of course, others from other places [such as Los Alamos, Rocky Flats, Brookhaven, Chalk River, Canada and Harwell, England], and even later from Savannah River.[55]

At these meetings, we discussed the urgent need for human data. I did the earliest extensive studies in trying to calculate acceptable levels for, especially, the bone seekers, like strontium, plutonium, and americium, curium, and phosphorus. But we had very little data on living organisms. Some data on rats, dogs, and mice. When I made calculations for the first [MPC (maximum permissible concentration)] level for plutonium, I had to use three rats that had been experimented with by Joe Hamilton at Berkeley. At these, in the neighborhood of one hundred different meetings I attended through the years, staff meetings at the various sites, there were papers given mostly at each local facility—say, at Rochester, Berkeley, or Los Alamos, or whatnot [and many of them related to how humans responded following accidents with radionuclides and the urgency to have more data on the human response to high levels of radiation exposure].

The research they were doing in other places was discussed. We'd brought in people from the UK[56] and from Canada—always there from Chalk River[57]—to discuss all the data that seemed to be pertinent to our problems. Where do these radionuclides go [in the human body]? What effect? And now the military was getting involved, we were in the real war; I'm moving up a few years. Germany had surrendered, maybe even if you'd like, Japan has already surrendered. (Whoa—we're getting into the middle period!) They were very much concerned about the radiation syndrome in reference to humans.

I adamantly refused to consider any human studies. I think [Alex] Hollaender did exactly the same. I doubt that anyone else at the Laboratory would have any interest or any knowledge [regarding human guinea pig] studies. I think I can say it's extremely unlikely that anyone at the Laboratory had any part in human guinea pig studies. I'm sure that *I* did not, other than what we mentioned, which was inadvertently [when] we

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55 A Department of Energy weapons site in Aiken, South Carolina, that, during the Cold War, was the major source of tritium for hydrogen bombs

56 United Kingdom

57 in Canada

got mixed up in something we pulled out of [it] rather quickly. You can confirm this with Bernard and Struxness if you like. [Later I tried to get Dr. Sweet at Boston General Hospital to limit his human exposures to the MPC of uranium we were using at ORNL because the high levels he was using damaged the kidneys of his patients and compromised the data we were seeking.]

I had numerous visits from *these* military [officials] and others. The discussion went on at all these meetings, both meetings on the stage [in panel discussions] and maybe in a bar, when we would relax. After a meeting that lasted eight hours during the day, we might sit down and have a beer. We would discuss various studies that were going on throughout the world. I remembered, in one case, somebody had read a paper from the Soviet Union. (I can't translate Russian.) They found that plutonium has a fairly high concentration in the gonads [so we considered lowering the MPC value for ^{239}Pu by a factor of 100 for genetic reasons].

Now we're in the middle of the Cold War, and our real enemy was the Soviet Union, and we thought that maybe the Cold War might spread to Austria, Hungary, and Poland. [If you were a civilized soldier], you didn't want to destroy those beautiful cathedrals and places of ancient art, and some places there go back in early history, which I won't go into. So the conversation was radionuclides[or] waste fission products seem to be the answer [for killing or encumbering people while sparing buildings].

Not only for that, but for security: to know what the Soviets were doing. We did some studies, and we did calculations on what you could find in the rivers, lakes, and oceans to determine the kind of reactors they had. What was going on with their neutrons, their uranium separation plants? It's remarkable what we could get from scattered pieces of information.

For my part, the most concern was the radiation syndrome.[58] I did something I'm very embarrassed that I did, and I'd never think of doing again. I consented to do studies on monkeys and baboons. So we conducted a long series on those primates, but never on *homo sapiens*—I never would even *think* about that as a possibility. I knew what had been done on prisoners, using radium and things of that sort that I'd heard about, and some suggestions of what maybe Joe Hamilton and cohorts might have done at Berkeley. I think I can speak for my deceased friend Alex Hollaender: he and I would never even think of human studies. There is no one else at the Laboratory that I think would even be concerned about or interested in it or have any knowledge of such studies [on humans].

My visits to Dugway and my discussions with these people, [were spent] trying to find out, for example, "If we dropped a nuclear weapon over some city in the Soviet Union and the plane didn't quite get out of the

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58 the biological effects of excessive exposure [greater than 100 rem (or 1 Sv) of penetrating radiation]

fireball area, the cloud, could they get the plane back to base, or should they abort the plane and bail out and destroy the instruments on the plane?" [Or,] "If we drop these fission products over a city in the Soviet Union, how soon afterwards could our troops get in?" and "Suppose they got ten rads"—now we're up to rads from roentgen, we're not [chronologically] at sieverts and so on yet—"If they got ten rads, could they march back to base, or could they drive the Jeep, could they operate this bulldozer and this other equipment?"

There was very intense interrogation of myself in particular, in health physics, on what could be done. I'm sure Hollaender and others, and many in his division [(Biology)] were questioned on this. I had a few biologists [in my Health Physics Division], but I don't recall now what [the military people] may have discussed with them.

Here we are with this pressure, tremendous pressure, to get information [that could be used for tactical and strategic radiation warfare]. "We're spreading these fission products out over, say, twenty square miles," [the military officials would say to us;] "There's a beautiful cathedral in the center here, but the Russian troops are all completely in the area. We want to get rid of them." Or maybe, "There is a plant producing nuclear weapons, that's [at] full production. We'd like to run those guys out with a little longer-lived radioactive material."

So we discussed the use of barium, lanthanum, and things of this sort. We were shipping large quantities [of ^{140}Ba and ^{140}La] to Los Alamos at the time. So we knew that [option] was available, up to at least one hundred thousand curies of barium and lanthanum. It was made-to-order because we had a large cross-section for production [of radionuclides] in reactors—even in our little baby reactor at Oak Ridge. So this pressure was on.

Atmospheric Releases of Short-Lived Isotopes Over Grazing Pastures

YUFFEE: Is this what led to doing basic rad warfare[59] studies at Oak Ridge?

MORGAN: No[, we never did any rad warfare studies in Oak Ridge]; I can get to that if you want to ask the question later. I cannot prove, but I would say in my mind there's 99 percent belief of certainty that the intentional release of over one hundred [thousand] curies at Hanford was part of this same program [to test the use of radionuclides in warfare].

YUFFEE: The Green Run?[60]

MORGAN: I had no proof of it. I knew many of the people there. I visited Hanford many times. I've attended these meetings. I had scores of meetings with

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59 radiation warfare, the use of fission-product radiation to kill enemy troops

60 For more on the Green Run Experiment, with an emphasis on its military purpose and the involvement of the U.S. Air Force, see DOE/EH-0455, *Human Radiation Studies: Remembering the Early Years; Oral History of John W. Healy* (May 1995).

military people. Francis Davis and Paul Reindhart in my group were the ones who developed this aircraft technique of finding sources [of radioactivity] when you fly over them.

Incidentally, Oak Ridge is accused of spreading radioisotopes in the area and flying over them. Well, what we spread was solid sources, of course, that were carefully taken out of lead containers that weighed maybe a half a ton to two tons. The lid was taken off [the container] with a backhoe device,[61] like a bulldozer, and the source was lifted out very carefully and set on the ground in a place that was marked with red stripes or white stripes. Then over here *(holds out his arm)*, three hundred yards [away], was another white stripe [where we] did the same thing.

Francis Davis and Paul Reindhart flew over these areas with the instruments they had developed, and in so doing, they developed this technique, which became so useful to the DOE.

In the earlier days, I used to have friends in the Department of Energy. I doubt if I have a single one there today, except maybe the director [(Secretary O'Leary)] would not consider me as an enemy, maybe even a friend. I have not gone along with the cover-up [of the AEC's and DOE's sanctioning of human radiation experiments]. I've not hesitated, sometimes, to crack the closet [of secrecy] where you can sort of peep in and see the "skeletons" hanging there [the wrongdoings of the AEC and DOE]. At times I did two or three things that I should not have done. I did not intentionally lose my job at Oak Ridge [by going pubic with what we were doing]. I should have done it, maybe. One time I did not intentionally lose my job, but I think it probably was right because it would have divulged the trigger mechanisms of our weapon, had I done so. On many occasions, even at Oak Ridge, I was on one side of the battlefield and the engineers on the other with their monitors [or umpires]—[Al] Weinberg, [Eugene] Wigner,[62] or [Martin] Whittaker—in-between.

Anyway, in the very early period at Hanford, you'll recall, they had a terrible problem. [Manhattan Engineer District Commander General Leslie R.] Groves was pushing Hanford, "Get that stuff up to Los Alamos as quick as you can"[63]—the cooling time[64] maybe now is a hundred days—"Let's get it down to fifty, maybe even forty," since they were taking all sorts of risks. As we all know, there are a number of radioisotopes of iodine [that have] half-lives much shorter than the 8-day iodine-131 we all hear about. Some of them have [half-lives of] days and hours and minutes and seconds. Then [if they reduced the

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61 an excavating machine with a bucket attached to a hinged boom that digs by being drawn toward the machine; invented in the first half of the 1940s

62 Eugene Paul Wigner (1902–95), U.S. physicist born in Hungary

63 where it would be assembled into nuclear bombs

64 time allowed for the short-lived fission products to decay away so that the fuel rods could be chemically processed and plutonium separated out

cooling time of the uranium slugs before they were dissolved in acid, the plutonium could be delivered earlier to Los Alamos (and please General Groves) but much more radioiodine would escape into the Hanford environment. Too often, Groves won in this contest and gravestones were erected in the cemeteries at an earlier date].

Developing a Chemical Dissolving Process to Remove Iodine From the Irradiated Uranium Slugs

MORGAN: But anyway, the temptation was to get this stuff to Los Alamos as quick as you could. That meant, then, releasing [from the facility] clouds of iodine and other radioactive materials of relatively short half-life; and the one that was most well-documented, of course, was iodine-131[, with an] eight-day half-life, roughly. At our staff meetings and meetings I had when I visited Hanford—I visited there many times other than the staff meetings—we had a great problem of how to get this iodine out [of the stream before venting the offgas to the outside air] and prevent if from getting out into the environment.

In retrospect, some things they did sound ridiculous; they even used water showers, like running [the offgas] through showers. Of course, that took a lot of it out, but not very much [to make a difference, from a public-safety standpoint]. [The radioiodine] would have occluded[65] on water drops [in clouds] and come out [to the ground the next time it rained].

They tried limestone, and silver worked real well. They used [silver] in different forms, but you'll recall at Oak Ridge [the] Y-12 [facility] used most of the silver from the U.S. mints, in their initial electrical conductors. Now, they needed even more than that at Hanford. Just think of the cost! And when you're in a hurry to get this in the right form, what you want is to make your [absorbing] area [as large as possible]—ideally you'd want [the airborne water droplets and your silver absorption material to consist of] small, round particles, where your ratio of area to volume is at a maximum.[66] You see, if your radius is close to zero, your area[-to-volume ratio] approaches infinity—theoretically, of course. When you put silver in that form, it'll go right on out [into the atmosphere when you vent], and you'll be throwing away all the silver in the mint! So there were a lot of practical problems using silver.

Cryogenics[67] worked nicely, but are [we] going to wait months to build a great huge cryogenic plant to do this? Well, that was out of the question.

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65 become incorporated, as by absorption

66 Photographic film manufactures strive to create silver iodide crystals that are flat and disklike, to expose more surface area to light. In the same way, Morgan and his group hoped to develop disklike metal particles, whose ample surface area would trap more iodine, which would fall to the chamber floor, fixed to the metal disks, where it could be safety disposed of.

67 the use of extremely low temperatures

Finally, it turned out, as I [recall]—and this is all recollections, I might be wrong—they finally ended up with copper in certain physical forms, which worked very well. That partially solved the problem.

I'm not saying that in the chemical-dissolving facility, they took all the iodine out, but they reduced the releases per week from maybe a hundred thousand curies to maybe only ten, twenty, fifty, getting down into what they considered a lower range. That was unavoidable [(releases of iodine)]: We had to do something about that maniac in Europe [(Hitler)] and win the war. So we had to get this plutonium to Los Alamos [even if we cause more thyroid carcinomas in the downwind populations].

And they were already finding out problems with the plutonium weapon, you remember. You can't use the cannon technique we used with uranium. (It's nice to be able to talk [about these] things without [having to show] your credentials!) At that time, we knew that we could not use the cannon device at Alamogordo.[68] At Alamogordo, and later, unfortunately, at Nagasaki [we used an implosion device].

These unfortunate releases took place, and I was not too concerned because I was interested in winning the war. I was willing to risk a possible few lives or a thousand cancers, to keep Hitler from dropping nuclear weapons on New York and Washington, Chicago, and eventually on Oak Ridge. I had my fallout shelter, and so did most of my senior scientists in the division. Though I was concerned [about] this fallout and [about the pressing need] to get the plutonium to Los Alamos in a hurry, I was not too concerned [with the thyroid cancers if this helped us win the war].

But later, when I heard of the intentional release that [had taken] place at the times when I visited Dugway, and when I had these visits [from military officials] about how beautiful it would be to use fission products concurrently with chemicals[, I was very upset]. In my mind—I can't prove it—the only reason for releasing the hundred or more thousand curies of iodine later on was to answer this question that had been proposed to me, [i.e., is it feasible to use radioisotopes as an adjunct to chemical warfare]. I see no admission of this by the military or by the DOE, or any records I've seen, but in my mind's eye. To change it, you'll have to show me some hard data to the contrary.

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[68] referring to the site of the first atomic bomb explosion, July 16, 1945, in the New Mexico desert. Alamogordo is a small community 50 miles southeast of the test site.

Plans Laid for Atmospheric Releases of Radioisotopes

CAPUTO: We've learned that the main reason for the 1949 release of iodine at Hanford was that Russia had just exploded "Little Joe,"[69] and the military wanted the means to figure how we, [the U.S.], could know what Russia was doing. We had to be able to analyze fallout to know what Russia [was up to], and that was the main purpose behind the Green Run.

MORGAN: Well, of course you don't take [to] court [what I'm saying] as fact. [I can't prove the Green Run was to find out the feasibility of using radionuclides as an adjunct to chemical warfare, but I have many reasons to consider this as fact].

I think they were planning this all along. Herb Parker might have been at Dugway when I was there, I don't know. He probably was. But I know there was great urgency to get the information I've just discussed. I know about what you just mentioned here, and that's, of course, what would be nice to release as a cause. I hope you won't take that as necessarily something that blots out what probably was the real fact.

YUFFEE: There was nothing similar to a Green Run at Oak Ridge? Was there a release of the same, not necessarily the same magnitude, but of the same type, at Oak Ridge?

MORGAN: Everybody was pretty cautious about putting anything in writing on this [touchy subject]. So, the last [court] case I testified in was [former Secretary of Interior, presently a practicing attorney, Stuart] Udall's case in Las Vegas. In the courtroom they had everything I'd written and all the reports, standing over 25 feet high; they had these long boxes, *(spreads his hands, palms in)* so long, *(stretches out his arm, palm-down)* so high. There were two [stacks of boxes] that went entirely to the ceiling of the things that I've written, of my reports[, publications, letters, etc.]. I was amazed at all I had written through the ages of the past.

Now, what is your question?

YUFFEE: The question is: Was there a Green Run type of release at Oak Ridge? We found some documents that suggest there might have been an intentional release [to the atmosphere] at Oak Ridge, but we're not sure, because of the way it was stated, if it actually was [only] a proposal.

MORGAN: I knew this was *discussed* with me and others, but [an actual release to the atmosphere—]not over my dead body! It was possible that Wigner or Weinberg might have [had such discussions and] Whittaker might have consented [to work toward this objective]. I knew [all the] parties [(Wigner, Weinberg, and Whittaker)] personally. I don't believe they would condescend to [an intentional release]. It would be condescending

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[69] In August 1949, the Soviet Union detonated a nuclear device that U.S. authorities subsequently coined "Little Joe." The United States responded in part by deciding in 1950 to advance to the next generation of thermonuclear weapons, fueling the country's need for tritium. Little Joe also necessitated the creation of a monitoring program to determine the design of other countries' nuclear weapons by analyzing the content of radionuclides present in fallout from their weapons' tests.

to carry out such a study using our own [people as guinea pigs]. In this case [at Hanford], when it wasn't our own—another nation of people, American Indians, as guinea pigs [made it even worse]. I know [that Biology Division director Alex] Hollaender would not, and I can say: I know Weinberg and Wigner would not. I know Weinberg personally and professionally, though we've fought over some issues in the early years. I knew Eugene—the late Eugene, who's being buried today or yesterday—very well, not just at the Laboratory but in many meetings and other discussions. I think I can say there'd be a 99.99 percent probability that they would not consent [to] or condone such a study in which the Laboratory had a major part.

YUFFEE: Moving away from the discussion of intentionally releases to rad warfare, in particular in the ARUU,[70] you did studies in 1948, with radiolanthanum and tantalum in particular. Maybe you could talk a little bit about those.

MORGAN: I never had anything directly to do with radiolanthanum.

YUFFEE: What about with tantalum?

MORGAN: Only discussions; I never took part in any studies.

YUFFEE: Am I right in calling it the ARUU program? Was that what the name it?

MORGAN: That's what I recall.

YUFFEE: You didn't take part in it?

MORGAN: Only on pen and paper; maybe some reports and letters and discussions. In fact, since I have no recollection of taking direct part, maybe you could refresh my memory and what the main motive of that study was.

YUFFEE: That's actually what I was hoping to get at by talking to you.

MORGAN: I remember the code name and all. You see, we had hundreds of those codes, and it won't come back to [my memory banks].

Unintentionally Widespread Dispersion From Phosphorus-32 Atmospheric Releases

YUFFEE: But do you have any other recollection of rad warfare research or tests that were done?

MORGAN: Well, going into ^{32}P and Bernard's work and Francis [Davis]'s. Francis is deceased, but if you are interested in the claims of many people, the sources put out to fly over. If you want to follow up on that and get the raw facts, you ought to talk to Paul Reindhart, who lives outside of Oak Ridge. I know, because he goes to the same church we go to. He used to be one of my students when I taught at Lenoir-Rhyne; I know him quite well. He could tell you details and all the early development of aerial surveys and anything that was done in Oak Ridge in that respect.

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[70] in reference to a single-source radiolanthanum test program at Oak Ridge National Laboratory. See CIC documents # 707033, 707034 & 707689.

When I covered the Windscale accident, for example, there were two things that Greg Morley and others impressed on me, where they made mistakes. One was, they didn't have an information center where everybody could come [with a request for information]. The [radiation surveyors had no place to interface with] news reporters, and they didn't have TV then but radio reports, [no open area where] radiation surveyors could come and bring their data and collate and disperse it appropriately. They had to come into the controlled area to get information and disperse it.

The other thing was that, initially, when they had this fallout [in the United Kingdom, it was several days before they got their light aircraft airborne]. I could talk for hours on that accident. They put their Geiger counters against these five-gallon jugs [of milk to take readings]; you remember: [those jugs] came up with a little lip on top. [They] put their Geiger counters on the side [of the jug] and if you had too much [(too high a reading)] they'd pour the milk out on the ground [so no one would drink contaminated milk].

But [the British] found, three days later, when they got their light aircraft airborne, that the radius distance [in which the milk was excessively contaminated] should have been twice as large. The area would increase by a square [so the area of dangerous milk contamination was four times as diffuse].[71] The mothers out there, justifiably, were frantic to hear, "My little baby has been drinking this contaminate milk! Contaminated, maybe, with strontium and cesium and iodine, and we weren't even told about it! Now, they're pouring all the milk out and will not let us get near the jugs that [held] the milk!"

So I brought [news of] this [miscalculation] back to Oak Ridge, and immediately we set up corrective measures. We set up an information center outside the control area. I had quite a number in my division [who] had private licenses [to pilot] small aircraft; I used to have one. I only flew about 80 hours solo. We [could] get our planes up in a half-hour [during our drills].

To show how stupid the AEC was, they should have taken my reports and reports from Windscale, and gotten [them] in the minds and the operations of all operators of [nuclear] reactors throughout the country. Even an order of magnitude more stupid than the DOE, even beyond the present regime, is the Nuclear Regulatory Commission [(NRC)].[72] I can't even think about them without thinking how deceitful, and dishonest they've been in some cases where I've been involved! I can prove that verbatim. Well, anyway, it's a shame that Three Mile Island operators did not know about [my reports and], did not take any of these mea-

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71 Since the area of a circle is πr^2, the area increases as the square of the radius. Hence, by doubling the release-point–to–pasture distance from 5 miles to 10, the crew could have spread the fallout over 314 square miles (3.14×100) instead of 79 (3.14×25), effectively diluting by 75 percent the dose reaching the cows.

72 the Federal agency that regulates the safety of commercial nuclear power plants

sures. They made all of the mistakes that were made at Windscale. We corrected them at Oak Ridge. Why did not the AEC—why did not the NRC get out this information [about how to prevent a recurrence of the Windscale accident]? They could have prevented that accident [at Three Mile Island].

Influence of Secrecy in Decisions About Radiation Exposure

MORGAN: If I had time I could discuss another accident, potential accident. Almost identical to Three Mile Island; it just didn't happen. Luck was in their basket, or whatever it was. [There were] other cases where we came to very-near accidents. They could have been [catastrophic], [but] in most cases were completely avoided. In others, they got out by a hair. Windscale was tragic, and of course Three Mile Island was terrible, but they all could have been avoided except for Windscale, if they had gotten this information out. They didn't do it. They just sat on their hands.

CAPUTO: So the AEC tried to keep the veil of secrecy, the same as the states?

MORGAN: Secrecy is more important than the lives of our citizens! They tried to imbue that in some of us, and maybe, to a little extent, to some of my friends at Hanford. I was willing to lose my job at Oak Ridge [before risking unnecessary exposure to employees or members of the public]. I think as long as I was there, this attitude [of radiological caution] prevailed, and the workmen would not dare go on the job of any kind unless my Health Physics surveyors were there with their instruments. The workers trusted them almost as much as they did when they prayed to their Lord at night. They wouldn't dare dig this trench, or go near this pipe, or go down this ladder, or go into this area. If the yellow ribbons were around [an area], they would never go near that unless the health physicist was there with his appropriate instruments.

Advice for Disposing of Tritium Safely Rebuffed by NRC

This attitude [for radiation safety] did not prevail, apparently, at some other places. Certainly it did not at Three Mile Island. Since I left the faculty at Georgia Tech, I've testified in over a hundred-fifty cases, trying to help people that have allegedly been injured by radiation. We won the Karen Silkwood case and Crumbeck case, and I was a sole witness [for the plaintiff when] we won in the Three Mile Island class-action suit.

But in most other cases, the AEC and DOE [have] called—[what was] then the Department of Justice [(DOJ)]; let me call it the "Department of Injustice" [to make false claims about radiation exposure] under some of the people there. They [(the DOJ employees)] actually *bragged* about the fact that they set up courses to train health physicists and lawyers on how to keep injured parties, injured from radiation, from getting any benefits! One of these was even held in Washington. I didn't attend it, but I can point to some people that attended the lecture that [Don] Jose from the Justice Department gave. Imagine: the Department of Jus-

tice—which is supposed, according to our Constitution, to provide justice to the citizen—training lawyers and health physicists how to cheat the public! How to allow people to be used as guinea pigs rather than be a hindrance to some nuclear or military program!

I more recently have fought the NRC on the release of [radioactive] hydrogen [to the surrounding air]. All the people in the cities of Washington[, DC] and Philadelphia and Baltimore are guinea pigs, as you well know. I gave [the NRC] a letter [from me], showing how you can get rid of this tritium [that had contaminated the cooling water at Three Mile Island] in a safe manner.

But they used the most nefarious techniques to throw out my testimony, and they would not even hear it, until I went—at my own inconvenience—to Washington to testify. They accused me of being a crook when I sent the letter to them, showing these other ways that they could release this tritium from Three Mile Island in the water because there [was an unidentified summary attached to one of the enclosed references]. [There were] hundreds of thousands of curies of tritium; [my letter explained] how they could dispose of it, in a relatively benign manner.

But they didn't want to hear that. They would rather use you darned subjects in Washington and Philadelphia as guinea pigs. So they boiled [it away into the surrounding air], and you breathed it.

Tomorrow I have a visitor from the UK, and I'm sure I'll have other interruptions in writing my book. If people could wait and read my book, they'd get some of the facts that you'd like to have now, maybe.[73]

But anyway, I sent in my letter [to the NRC] showing these other ways you could dispose of the tritium without using humans as guinea pigs. It would be a little more expensive, a little more trouble, but the exposure would be almost zero. I also indicated, as I recall in the letter, why tritium is far more hazardous than was agreed-to officially when I was chairman of the committee[74] that set the standards for tritium and other radionuclides for 20 years, you see. So I wasn't just talking out of the wind.

I sent this in, but I felt maybe I'd better send some backup information. So I grabbed, out of my library, a few folders and things and put them in a big envelope and sent them in. Inadvertently, one of these documents from the UK had a page on the back that summarized the document. It may have had some other information; but it wasn't supposed to be there. They accused me of being a crook, because this document had this [summary] attached [as] the last page! They didn't even go into

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[73] Morgan adds: "This visitor from the UK has sent me reports showing the present MPC values of tritium, ^{3}H, are too high by at least a factor of five. This visitor is Ian Fairline from St. Bartholomew's Medical College."

[74] the Internal Dose Committee of the International Commission on Radiological Protection (ICRP)

any of [the scientific information I sent. They were very cruel and most hostile].

An advisor to the—I don't know his position, I guess the court recorder there, or maybe the second stage of lawyers they had—was Dr. [L.S.] Taylor, who had been chairman of NCRP for many years, who [now] is on the other side of the fence. He used to be a very close friend. I don't consider him an enemy now, but I disagree with him [vigorously] in his position. He was there advising them what to do—the only scientist there advising [what accusations to make]. But these twelve judges—who I [had] helped to select a year earlier, because I had been one of those to help select the judges of the NRC—they threw out my testimony and decided, "Well, it's a lot cheaper to use you [people downwind] as guinea pigs." I guess they're through [sending this tritium into the air]; they've completed their [guinea pig] study on you folks, if you were [living] there [in Washington, DC].

They saved a lot of money in this decision. Tritium can cause leukemia as well as solid tumors, and the leukemias come in early, some even beginning in five years, the midpoint about 15 years. Some dribble in even as late as 30 years; but [for] solid tumors, probably the midpoint on them is 30 [years].

Chairing the Public Health Fund (1980–92)

Having testified on the Three Mile Island case as the only witness [for the plaintiff], I was asked to be chairman of the Public Health Fund. You might want to get hold of some of their documents. The decision of Judge Rambo [in our favor] has added up, with interest, to over 15 million dollars, which were used for research [on the effects of low-level exposure to ionizing radiation].

I was asked to be chairman of the so-called Public Health Fund by the Berger law firm in Philadelphia.[75] We worked on that for a period of over ten years, following the research and administering this money. The program of which I was most proud is that of Dr. Alice Stewart, who is a wonderful woman of high intellect and integrity, with great skill in epidemiology[76] as well as medicine. She's done more extensive epidemiological work than any other person that's ever lived—living now or dead. A portion of our money [went to her research]; I think it's added up to about two million. She has independently studied the film badge records and carcinogenic[77] rate, [as determined] from death certificates in this country [of former employees of the X-10, Y-12, and K-25 facilities of Oak Ridge and former employees of Hanford, Los Alamos, Savannah River, Rocky Flats, etc.].

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75 Berger and Montague, P.C.

76 the branch of medicine dealing with the statistics of incidence and prevalence of disease in large populations and with detection of the source and cause of epidemics; *also:* the factors contributing to the presence of absence of a disease

77 relating to substances or agents that tend to produce cancer

I haven't heard anything from that committee [(the Public Health Fund Committee)] now for over two years; I think maybe I said the wrong thing at one of our last meetings, in which I said I was more interested in getting the facts than I was in getting the data to the judge. That doesn't go over very well with lawyers. Anyway, I think that independent studies were in order [for we found an excessive cancer risk at the so-called permissible exposure levels].

Of course, you know of the case where we spent ten years before we could get the raw data from the AEC or the Department of Energy. We finally got it. Freedom of Information[78] would not work. Finally got it from testimony we brought in from a man [(Dr. Greg Wilkenson)] who worked at Los Alamos and then down in Texas. In the court hearing in Columbia, South Carolina, he pointed out that [when] he was working at Los Alamos studying the data from the Denver area, he found significant increase in cancers, [but] that he wasn't allowed to publish his data. I could look up in my notebook and get his name. That's one of the problems you've [mentioned] you run into, after you're 70 or 80 or approaching 90 years: you always remembered all the names of the millions of people and I can't think of his name at the moment.

Vanderbilt University Study of Pregnant Women and Iron-59 (1945–49)

YUFFEE: I have one question, a specific question that I probably should have asked you when we were back on this subject. So I'm going to go back a little bit. Do you remember a Dr. C.W. Shepherd?

MORGAN: Oh yes, I do, from Vanderbilt [University in Nashville, Tennessee]. I was for many years [associated] with [Dr.] Francis Slack, who was chairman of the Physics Department there. I was an adjunct professor there; I don't recall [for] how many years; I would guess at least a decade. So I knew him, and I knew [Paul F.] Hahn at the time; I knew a lot of others [there]. I remember those two, specifically.

YUFFEE: Were you familiar with the Vanderbilt study with pregnant women and iron-59?

MORGAN: I know Paul Aebersold was very interested; and Paul and Mickie, his wife, were good friends of my wife and me; we both belonged to two dance clubs. So I knew that they had a study using iron-55 and -59. I was somewhat appalled at the time, and I think even Paul was a bit worried that they were using the, I guess it's the -59 that is the more dangerous one you should not use. I even then made calculations showing—well, I'm sure I did, because I made hundreds and hundreds of thousands of calculation on the different radionuclides. The risk in the shorter-lived radioisotope [(iron-59)] was more hazardous than the

[78] the Freedom of Information Act of 1974, which entitles U.S. citizens to see Government documents that would otherwise remain privileged or classified

longer-lived one.[79] I won't go into the forty or fifty reasons why [but mostly because of a much higher energy emission per disintegration]. That meant that you should not use reactor-produced radioactive iron. That is, you should not put stable iron in the reactor, and cook it up with neutrons to make these two isotopes, and use a chemical extractive of this for any studies of blood or anything else [with humans].

My impression—from casual comments of Paul Aebersold—was, I'm afraid, that Hahn, Shephard, and the others there, whom I can't recall, used some reactor-produced iron, which was a terrible choice. Why would you knowingly give doses more than [10 times than necessary in a human study? This should have been of great concern] when you're giving doses that were right at the occupational level and it would go to the fetus, and we all knew the placental barrier would not take out iron? In fact, you wouldn't have a fetus if it did. Here we were, exposing man [as a fetus] at the most critical stage [of his development]. We already knew that the fetus was more critical a human being than I was, Hahn, or Paul Aebersold, and here they didn't even bother, apparently, to cook up in special preparation in a reactor and pay a few, maybe twenty or fifty thousand dollars, for their material rather than get it free from Oak Ridge or wherever they got it.

YUFFEE: That was my question: Do you know where they would have gotten it?

MORGAN: No, I don't. Paul, I'm sure, had detailed records. [But I was always under the impression it was gotten from Oak Ridge].

Difficulty Obtaining Historical Information, Despite Freedom of Information Act

MORGAN: When I was at Oak Ridge, there was constant pressure to shred data, especially the film badge data, but I fought against it. If you go there now and you have the same luck I had—I have to go through two locked doors to get to a librarian, who doesn't know where anything is, to help me find reports that *I* wrote that *weren't* classified, that *I* sent out [all over the world] to hundreds of people back in that period.

This is Martin Marietta[80] that your agency is supporting, supposedly to help the public. They have offices there for other people that worked there. When I left Oak Ridge [as a Federal retiree], I did not get an offer of as much as a dollar a year for a job [at ORNL, as some of the retirees did, because I was never a yes-man]. I would not have accepted, had I [received such an offer, but did not have the pleasure of turning it down]. I had offers of jobs in Washington and other [similar] places. I did not want to get into that den of thieves. I wanted to be in the South; furthermore, I wanted to do research [and teach again in a university].

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79 Iron-55 has a half-life of 2.94 years; iron-59, 45.1 days. Unlike iron-55, iron-59 emits beta and gamma radiation.

80 Martin Marietta Energy Systems (now Lockheed-Martin), the prime contractor for Oak Ridge National Laboratory

I ended up at Georgia Tech. Maybe that was a bad choice. I had offers at my former school, [University of] North Carolina; I wish, in retrospect, that I'd gone there. I think it became a decision of Nuclear Power [versus] The Health of People—and Carolina would have been a much better choice than Georgia Tech, who wants to please Coca Cola and the nuclear industry.

I've given a black eye here—you'd better tell your director [(Energy Secretary O'Leary)] to watch out, because I can name four women that have been killed since they took strong positions against nuclear power, beginning with Karen Silkwood, and I testified in her case. It's very dangerous for—especially a woman, for some reason. You'd better be careful. I don't know why they pick on women.[81]

CAPUTO: *(smiling)* We [(women)] must learn more [of their secrets]—we're more dangerous.

MORGAN: I doubt she (Secretary O'Leary) could live through this (*inaudible*) Administration because some of the old (*inaudible*) are back in the establishment.

YUFFEE: I don't have any more specific questions.

CAPUTO: Neither do I.

Have we missed something? Is there a question we should have asked you?

MORGAN: I don't know quite what you have in mind. As I say, I don't want to give you everything that will go in the book [I am writing jointly with my lawyer friend Ken M. Peterson of Wichita, Kansas].

CAPUTO: What's your book about?

MORGAN: It's about the early history of health physics. But, as I said, I have to depend on my memory, mostly, and my library, which is incomplete, because Martin Marietta won't allow me free access in the Laboratory, [a privilege they extend] to scores of other former employees. They are afraid of me because they think I'm against nuclear energy, I guess.

I'm not against nuclear energy; I'm for a long-range study [of nuclear energy]. I just think that Admiral Rickover[82] was just too successful, and we land-based his PWR[83] too soon. We should have done at least thirty more years' research before we built reactors on land, and certainly within 50 miles of big cities like New York and Philadelphia. I testified

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81 Morgan adds: "I have the highest admiration for your Secretary O'Leary for the brave stand of openness and honesty she has taken, but I have a sense of uneasiness. I testified in the Karen Silkwood case and know of these heroic women who have suffered the same fate as Karen. I fear she has a bear by the tail."

82 Hyman George Rickover (1900–1986), U.S. Navy admiral, born in Poland; helped to develop the nuclear submarine and is sometimes called the "Father of the Nuclear Navy"

83 pressurized water reactor—one of the two kinds of light-water reactors used in virtually all domestic commercial nuclear reactors. Actually, U.S. Navy submarines rely chiefly on the other kind: boiling water reactors (BWRs).

against the one near New York,[84] as you probably have found out. So, though your director has asked for free access to information, Martin Marietta hasn't come through. I don't know whether GE and Westinghouse and the others[85] have done so [at other DOE operations]; I doubt it.

If a peon like [me,] someone way down the line like I've always been, someone that has a lot of facts about what actually went on, [can't get in], I doubt if they would let [you] into these facilities. Even though [I] may still be for nuclear power [it's in the hope and confidence nuclear power will eventually find its proper place on planet earth as it has in the rest of the universe]. When I say "I'm for nuclear power," I'm for research for getting rid of some of those bugs [that have caused its failure].

Studies on Nuclear Waste Storage Issues

MORGAN: When I was director [of the Health Physics Division of] Oak Ridge [National Laboratory], we did all the advanced research on high-level waste disposal. We did the studies in the salt mines in Kansas. We developed big machines to carry the sources in [and out of the mines], and we looked into the Wigner effect,[86] the storage of energy by the electrons and the positive ions [in energy] levels in the salt. These are what caused the accident, as I said earlier, at Windscale and [contributed to the] Three Mile Island [accident]; and we looked into the transport of moisture in different chemical forms going through the salt and lots of things [of this nature]. I think the Swedes have made a little more progress, since that time, with storage [of high-level nuclear waste] in granite.[87]

But when they talk about encasing it in gold, I balk. Not because the gold isn't worth anything compared to the problem. The problem is, in storing, we had to always think about, "What's the value of this?" Would people a thousand years from now inadvertently go into this and try to retrieve that gold or silver or whatever you encase it in? [If so,] you can't dare use it.

Or you can['t] put [it] in some [geological] formation that itself will be of great value. Well, salt, certainly sodium-chloride–type salt,[88] is of essentially no value. I can set up a factory out here and have the means to get enough salt to supply the world for all time from the Gulf of Mex-

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84 Shoreham, the Long Island, New York, nuclear plant that operated for only a few days before being shut down because of safety concerns

85 the other prime contractors operating Government-Owned, Contractor-Operated (GOCO) facilities that comprise the DOE's former Weapons Complex

86 Wigner Force, the short-range nuclear force of nonexchange type postulated by physicist Eugene Wigner as part of the interaction between nucleons

87 Nuclear waste from Swedish commercial nuclear reactors is encased in special copper-clad glass capsules, which in turn are stored underground in stable granite formations.

88 NaCl, ordinary table salt

ico. But there are cheaper ways of getting it than getting out of the ocean, though it's done commercially [that way] in many places.

So, [at] Oak Ridge, the whole time I was there, there was pressure on the engineers to move all engineering studies out of [the] Health Physics [Division]. We had some of the best engineers, the best physicists[, etc.,] in the Laboratory. I think [Sam] Hurst and [Rufus] Richie both have recently graduated from there. They should have Nobel prizes for things they've done sponsored by the Department of Energy. So it [(DOE)] has done some good things in the past.

Anyway, all the time we were there, there was tremendous pressure from the Chemical Technology Division to take that away, because "Health Physics has no business doing this." Immediately when I left, things changed; shortly after Weinberg left, [conditions at ORNL deteriorated even more]. Of course, studies [on high-level waste disposal] were taken over by the engineers, and—surprise!—now they've discovered the wheel. Look at the progress they've made. So it goes. ❑